I HAVE been much interested in reading the above, and I hope that Mr. Hay's example will be followed by other observers, in order to establish a fair average result of the relative cost of the coke-gas grate and the ordinary coal grate. Mr. Hay's results are not as favourable as those obtained by myself, owing probably to some imperfection in his arrangements, which are not described sufficiently to form any judgment upon them. I should like to know, for instance, whether the copper-back plate, which I presume he used, although it is not referred to, was backed by fire-clay, or whether it touched the ironwork of the fireplace, whereby its heat would be conducted away. This alone might account for the difference in result obtained by Mr. Hay and myself, and I think this opportunity is a favourable one to send you the figures resulting from my own observation of the original grate described in my article in NATURE, vol. xxiii. p. 25. This grate has now been in use from November 8 to January 31, during which period it has been alight sixty-six days, and the average time during which a bright fire has been kept up has been eight hours daily. During this period of 528 hours there has been consumed—

	£	*s.*	*d.*
1112 lbs. coke at 18*s.* a ton	0	9	0
581 lbs. smokeless coal at 20*s.* a ton	0	5	2
4100 cubic feet of grs at 3*s.* 6*d.* per 1000 ...	0	14	4
	1	8	6

Or an average of 0·518*d.* per hour, instead of 0·525*d.*, as resulting from my first observation. The average consumption of solid fuel per hour has been 3·2 lbs., and of gas 7·7 cubic feet. The full supply of gas has generally been allowed during the first hour of lighting, after which it is turned down to about a third; this I find to be a convenient mode of working.

In comparing my results with Mr. Hay's, it must be borne in mind that my room has a capacity of 7200 cubic feet, with northern aspect, and his a capacity of 3000 cubic feet, also with northern aspect; his consumption should therefore be only $\frac{3000}{7200} \times 0{\cdot}518 = 0{\cdot}216d.$ (instead of 0·347*d.*), which figure would prove an economy in the employment of the coke-gas grate over the coal grate, which is 0·2785*d.* by his own showing, and would agree with the comparative results contained in my original communication. C. W. SIEMENS

February 2

On the Spectrum of Carbon

IT is very desirable that, if possible, some definite conclusion should be arrived at concerning the chemical origin of the bands, which Prof. Liveing calls "hydrocarbon bands," and the importance of the point at issue must be my excuse for again addressing you on this subject.

In my previous communications I pointed out that if it can be shown experimentally that the electric spark, in an atmosphere of cyanogen *free from hydrogen*, gives the groups in question (the groups δ and γ, wave-lengths 5165 to 5082 and 5635 to 5478 respectively, are here the only ones considered), they must be due to carbon, and remarked that the hypothesis that they were due to traces of hydrogen present as impurity is "to adopt an extreme hypothesis which must be supported by cogent experimental evidence before it can be accepted." Prof. Liveing admits the justice of this demand, and then goes on to say that such "cogent experimental evidence, so far as the relations of carbon and *nitrogen* are concerned, will be found in our complete papers on the spectrum of carbon compounds in the *Proceedings* of the Royal Society." This appears to me to be equivalent to an admission that—as concerns carbon and *hydrogen*—no such experimental evidence has yet been given; which is also the conclusion to which I came after perusal of the papers of Profs. Liveing and Dewar referred to.

It would seem then that the burden of proof that cyanogen exists in which the spark will not give rise to the bands δ and γ rests with Prof. Liveing. Nevertheless I have repeated the experiment with cyanogen, described in this journal (vol. xxiii. p. 197), so as to set aside the objections raised by Prof. Liveing to the former experiment. The apparatus was in this case constructed of one piece of glass—a long piece of hard glass tubing. This was carefully cleaned, the tube was then contracted at two points, so as to separate a short portion of the tube, into which platinum wires were fused, so as to form a discharge tube. The whole tube was next heated to redness in a furnace, while a current of oxygen passed through it for some considerable time. The greater portion of the tube on each side of the part containing the wires was then filled with phosphoric anhydride, and a short length of the tube, separated from the discharge tube by as great a length of phosphoric anhydride as the length of the tube permitted, was employed as a retort, and filled with mercuric cyanide; the other end of the tube was drawn out and dipped beneath sulphuric acid. The mercuric cyanide employed, after being finely powdered, was dried for a long time in an air-bath, then transferred to a clean hard-glass tube, in which it was repeatedly heated, while a current of air dried by passing over calcium-chloride and phosphoric anhydride was drawn over it. From this tube it was transferred immediately to the retort-tube. In making the experiment the mercuric cyanide was heated so as to give as slow a current of cyanogen as possible, which was continued long enough to expel all the air from the tube. The tube was then sealed up, leaving the discharge-tube, with a phosphoric anhydride tube on each side of it, and put aside for a week. The spectrum was then examined, with the same result as before. The tube gave a brilliant carbon spectrum, of which γ and δ (the positions of which were measured) were the brightest groups. No trace of the hydrogen C-line was obtained. Prof. Liveing objects that this is not a sufficient proof that hydrogen is absent (in which I cannot agree with him), and suggests that "a real test would be to see whether, when the spark gives the line-spectrum of carbon, the hydrogen-lines do not also appear." This test is however not applicable, since (according to my experience) cyanogen cannot be made to give the line-spectrum of carbon. Further, in this particular case the spark could not be got through the tube when the condenser was put on.

Giggleswick, February 11 W. M. WATTS

"Prehistoric Europe"

As there was no space to allow of all the authorities being cited in my criticism of the above work I now give those which relate to the facts called in question by Dr. James Geikie in NATURE, vol. xxiii. p. 336.

1. Dr. James Geikie repudiates as absurd the view attributed to him, that the palæolithic gravels "which overlie the chalky boulder clay of East Anglia were covered by an upper and younger boulder clay," and denies that he ever wrote anything which would justify that opinion. In "The Great Ice Age," 2nd edition, p. 531, he writes: "The palæolithic beds dovetail into the glacial drifts, and are overlapped (as in Yorkshire) by the deposits thrown down during the final cold period. To the last interglacial period then we must refer the great bulk of the palæolithic river gravels of the south-east of England." If this be true, where are the glacial deposits in question to be seen? If they ever were above, or "overlapped," the palæolithic gravels, they have, so far as our present knowledge goes, been utterly destroyed. Of course this view *is* absurd.

2. The reindeer associated with the hippopotamus and hyæna in the same stratum in the Victoria Cave was discovered while the exploration was under my management, and was published in Brit. Assoc. Rep. 1872, *Trans.* p. 179, and again in Mr. Tiddeman's Report, *op. cit.* 1876, *Reports*, p. 118. The animal is omitted by the author where its presence would destroy his argument as to climate, but he does not forget to record its subsequent discovery at a higher level, where it falls in with his argument. It may be remarked that the association of reindeer with hippopotamus in this cave has no special theoretical value, because the two animals have been found together in several other hyæna dens.

3. The fossil mammalia of Mont Perrier are typical Upper Pleiocene, as may be seen from the works of Croizet and Jobert, Gaudry and Gervais, and as I can testify from their examination. The glacial origin of the overlying tuffs, which I have examined under the guidance of M. Julien, seems to me to be open to considerable doubt.

4. The mammalia of Leffé, and those of the Val d'Arno with which they are classified by Dr. James Geikie, characterise the Upper Pleiocenes of Italy, as may be seen from the careful essays published by Dr. Forsyth Major, and from an examination of the magnificent collection in the museum of the University of Florence.

5. If pages 309-318 of "Prehistoric Europe," dealing with "interglacial epochs," do not imply a belief that the Neolithic skull of Olmo is interglacial, I am unable to ascertain their meaning.

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The questions whether a geological period is to be classified as hitherto it always has been classified, by an appeal to zoology or by an appeal to ice, and whether the naturalists who have devoted themselves to the study of mammalia have only "opinions," while Dr. James Geikie enjoys "the facts," may be left in silence to the judgment of geologists. In the review under discussion all reference to my own opinion and works has been carefully omitted. Here, so far as I am concerned, the discussion ends. W. BOYD DAWKINS

Owens College, February 11

Geological Climates

IN NATURE, vol. xxiii. p. 241, Dr. Haughton repeats his former statement that "it is imposible to suggest any rearrangement of land and water which shall sensibly depress the temperature of the east of North America." Now we must only look about us to see that the east of Asia is colder than the east of North America, parallel for parallel, and this especially in winter. The *mean temperature of January* is as follows in places situated as far as possible under the same latitude and at the same distance from the sea :—

Lat. N.	Eastern Asia.		Eastern North America.		Difference.
56½°	Ajan	– 4·2	Naïn, Labrador	– 3·8 ...	0·4
53	Nikolayewsk, Amoor	– 12·1	Rigolet, Labrador	– 1·1 ...	11·0
43¾	St. Olga Bay ...	12·9	Portland, Maine	21·2 ...	9·3
43¼	Wladivostok ...	6·1	Portsmouth. N.H.	25·0 ...	18·9
40¾	Newchwang ...	10·4	Paterson, N J. ...	26·6 ...	16·2
40	Pekin	23·7	Philadelphia ...	31·3 ...	7·6
31¼	Shanghaï	38·3	Mean of Savannah and Ft. Marion	53·6 ...	15·3
22¼	Victoria, Hong Kong	59·5	Habana, Cuba[1]	71·4 ...	11·9

This shows that (1) from lat. 20° to 55°, Eastern Asia is everywhere from 7½° to 19° F. colder in January than Eastern North America ; and that (2) those parts of the coast of Eastern Asia which are not separated by mountains from the interior lowlands are much colder than those which are sheltered, but even the latter parts, though relatively warmer, are yet much colder than the same latitudes in Eastern North America. These differences are explained by geographical position. Asia is the larger continent; its eastern interior is more secluded from the influences of warmer seas, and its eastern coast more subject to Continental influences, and thus colder in winter than North America. We thus see by the example of Asia that a colder temperature than in Eastern North America does really exist now in the same latitudes. The example of Eastern Asia shows us geographical conditions which tend to produce an exceedingly cold winter. We have but to look at the middle and higher latitudes of the southern hemisphere to see so cold summers that nothing of the kind is met with in the northern. I do not know on what authority Dr. Haughton states that the annual temperature of 32° F. is met in the southern hemisphere but on 62° 41′ S. We do not have observations during the winter in these latitudes, but the mean temperature of January (the warmest month) is found to be 35·2 on 60° S. and 32·4 on 63½° S. Or (by the observations of Sir J. Ross) the mean annual temperature can certainly not be less than 4½° below that of January, so that it would be not higher than 30·7 on the 60° S., and 27·9 on the 63½° S.

St. Petersburg, February 5 A. WOEIKOFF

Variable Stars

WITH reference to your remarks on variable stars in the Astronomical Column of NATURE, vol. xxiii. p. 206, I beg to send a few observations made by me (on some of the stars referred to) during the past few years :—

5. 35 Camelopardi. October 1875. I found this star about 6½ m. and fainter than *o* (27 Fl.).—October 6, 1879. 7 mag. ; about 1 mag. less than *o*.

6. Rümker's star. I have the following observations : March 27, 1875. About 7 m. ; fainter than 25 Monocerotis.—January 19, 1876. 6½ m. ; less than 25, but brighter than two 7 m. stars *s.f.* it.—March 18, 1877. Distinctly visible to the naked eye ; about 6 m., but less than 25 (5.6 m. Heis). The above observations were made in the Punjab.

7. 65 b^2 Geminorum. December 1, 1880. 65 so exactly equal to 64 Geminorum with opera glass that I could see no difference between them in magnitude.

8. 16 Leonis Minoris. March 27, 1875. About 7½ m.—January 19, 1876. 7·3 or 7·5 m.

10. Lalande 38405. August 31, 1877, I found this star fainter than Lalande 38388, which lies about 20′ north of it ; also less than a 6 m. Harding (Lalande 38214) *s.p.* it. Brighter than Lalande 38342 (7½, 8), which lies *n.p.* it.

11. 33 Capricorni. August 1875 I estimated this star as 6½ m. ; August 1876, 6 m., and slightly brighter than 35 Capricorni.

12. ι (17) Andromedæ. From numerous observations, beginning in May 1875, I have detected a variation in the light of this star to the extent of about half a magnitude. It is sometimes distinctly brighter than κ Andromedæ, and sometimes decidedly fainter.

With reference to β and δ Scorpii I find the following observation in my note-book :—

"Punjab, August 10, 1876. β Scorpii (2 m. Heis) and δ Scorpii (2·3 m. Heis) almost exactly equal. Perhaps δ, if any thing, *very* slightly the brighter of the two. J. E. GORE

Ballisodare, Co. Sligo, Ireland, February 5

The Mode of Flight of the Albatross

THERE seems to be a prevailing idea that the albatross in his flight is in some way "assisted by the wind." I think this is a mistake ; the manner is well known. The method I believe admits of a very simple explanation. His secret consists in his power of acquiring great momentum together with the large superficial area of his extended wings ; with scarcely a motion of his wings he will fly straight against a strong wind with a velocity greater than that of any racehorse ; this is inconsistent with the idea of his being "assisted by the wind."

In attempting to rise from the water (I believe he is unable to rise from the land or from a ship's deck) he flaps his wings violently to get his body out of the water ; at the same time, paddling rapidly with his webbed feet, he acquires a moderate degree of momentum, sufficient, with outstretched wings, to carry him forward and upward upon an easy incline. The case is similar to that of a boy taking a run with his kite string in his hand to give his kite a start. During this first rise he will generally give a few heavy, lazy flaps, and then stretch his wings steadily to their full extent ; now as he gradually rises he must of course as gradually lose his acquired momentum till it suits him to acquire more, when he may be twenty, thirty, or fifty feet above the surface, but a much greater distance from the place where he left the water, measured on the surface ; by slightly altering his position, by a movement of his tail, he takes a shoot downwards at any angle that suits his convenience, still without his wings outstretched. This is precisely the case of a boy shooting down a coast on his sled ; the propelling force is the same. The bird directs his course mainly with his tail, the action of which upon the air is identical with the action of a ship's rudder upon the water. By this downward motion, his velocity rapidly increasing, he acquires a degree of momentum sufficient to carry him up again to a height equal to or greater than that from which he started. In this up and down long wave-like motion, with all its variations on either side, consists the whole of his flight day after day for hundreds of miles ; at long irregular intervals he may give a few lazy flaps with his immense wings. Other birds use the mode of flight of the albatross, but to a smaller extent, for the reason, in the case of smaller birds that, the ratio of feathers to bulk being greater, their specific gravity is less, consequently they are unable to acquire the degree of momentum necessary to carry them upward ; but on the other hand they have the power of sustained effort in moving their wings rapidly, which the albatross has not. Gravitation then, which prevents him from rising directly on the wing, is the motive power of the albatross when aloft. He must always take a run or paddle over the surface of the water in order to get a start, and on the land or the deck he is a prisoner, because he has no water in which to paddle himself along with his webbed feet, and he is unable to run. Instead of being assisted by the wind, his speed is lessened by just so much as the wind's velocity, when it happens that the direction of the wind and his intended course are opposed to each other, but with the wind his speed is just so much greater than it would be in a calm.

I do not advance this explanation as an imaginative theory. I claim more for it. I have had many opportunities of studying the movements of the albatross for consecutive days, and I feel confident that the above will be found to answer all required conditions. HOWARD SARGENT

Cambridge, U.S.

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[1] Nearly one degree to the North of Victoria.

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